EXHIBIT 2

Public Opinion Quarterly, Vol. 83, No. 4, Winter 2019, pp. 723–748

VALIDATING SELF-REPORTED TURNOUT BY LINKING PUBLIC OPINION SURVEYS WITH ADMINISTRATIVE RECORDS

TED ENAMORADO*
KOSUKE IMAI

Abstract Although it is widely known that the self-reported turnout rates obtained from public opinion surveys tend to substantially overestimate actual turnout rates, scholars sharply disagree on what causes this bias. Some blame overreporting due to social desirability, whereas others attribute it to nonresponse bias and the accuracy of turnout validation. While we can validate self-reported turnout by directly linking surveys with administrative records, most existing studies rely on proprietary merging algorithms with little scientific transparency and report conflicting results. To shed light on this debate, we apply a probabilistic record linkage model, implemented via the open-source software package fastLink, to merge two major election studies—the American National Election Studies and the Cooperative Congressional Election Survey—with a national voter file of over 180 million records. For both studies, fastLink successfully produces validated turnout rates close to the actual turnout rates, leading to public-use validated turnout data for the two studies. Using these merged data sets, we find that the bias of self-reported turnout originates primarily from overreporting rather than nonresponse. Our findings suggest that those who are educated and interested in politics are more likely to overreport turnout. Finally, we show that fastLink performs as well as a proprietary algorithm.

TED ENAMORADO is an assistant professor of political science at the University of North Carolina at Chapel Hill, Chapel Hill, NC, USA. KOSUKE IMAI is a professor of government and of statistics at Harvard University, Cambridge, MA, USA. The authors thank Bruce Willsie of L2, Inc., for making the national voter file available and Matt DeBell of ANES and Steffen Weiss of YouGov for technical assistance. The authors also thank Benjamin Fifield for his advice and assistance and Steve Ansolabehere, Matt DeBell, Shiro Kuriwaki, Xiang Zhou, three anonymous reviewers, and audiences at the 2018 Annual Meeting of the American Political Science Association, the Institute for Policy Research at Northwestern University, the Center for the Study of Democratic Politics at Princeton University, New York University (Politics), and Yokohama City University (Economics) for helpful comments and suggestions. This paper draws from the 2016 American National Election Studies (ANES) and the 2016 Cooperative Congressional Election Study data. *Address correspondence to Ted Enamorado, 304 Hamilton Hall, University of North Carolina at Chapel Hill, Chapel Hill, NC 27514, USA; email: tede@email.unc.edu.

doi:10.1093/poq/nfz051 Advance Access publication January 31, 2020

4:22-cv-62
PX-182

The accuracy of self-reports is essential for ensuring the validity of survey research, and yet many respondents misreport or refuse to answer when asked survey questions that are sensitive in nature. Social desirability and nonresponse biases make it difficult to precisely estimate the prevalence of certain attitudes and behavior. A well-known example is self-reported turnout rates obtained from public opinion surveys. Figure 1 shows that the gap between self-reported and actual turnout rates has been consistently exceeding 15 percentage points over the last five US presidential elections.[1]

The self-reported turnout rates are computed using survey weights from two major election surveys, the American National Election Studies (ANES) and the Cooperative Congressional Election Study (CCES). The ANES has been conducted for every presidential election since 1948, whereas the CCES is a large-scale online survey that has been administered for every election since 2006. While the ANES has used face-to-face interviews, it also conducted an Internet survey in the last three general elections. The difference between actual and self-reported turnout rates is remarkably consistent during this period. While the actual turnout rate has hovered between 50 and 60 percent,

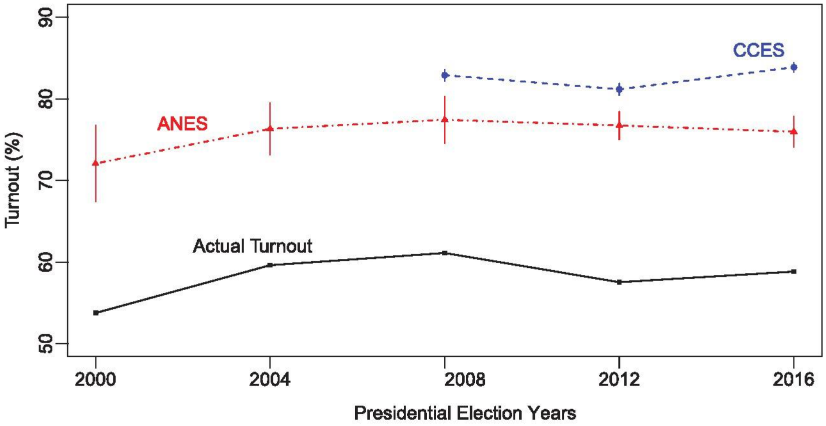

Figure 1. Comparison of actual and self-reported turnout rates. The actual turnout (solid line with squares) is computed using data from the United States Election Project (http://www.electproject.org), whereas the self-reported turnout rates are based on the American National Election Studies (ANES; dash-dot line with solid triangles) and Cooperative Congressional Election Study (CCES; dashed line with circles), using appropriate survey weights. The vertical bars represent 95 percent confidence intervals.

[1] The actual turnout is obtained from the United States Election Project (McDonald and Popkin 2001, http://www.electproject.org) and represents the turnout based on the population of eligible voters (see Online Appendix A1.1 for more details).

the survey estimates have always stayed above 70 percent, with the CCES exceeding 80 percent.

However, scholars sharply disagree on what causes the bias of self-reported turnout rates. Some blame overreporting due to social desirability (e.g., Silver, Anderson, and Abramson 1986; Bernstein, Chadha, and Montjoy 2001), while others attribute the bias to nonresponse (e.g., Burden 2000). Although in earlier years the ANES validated self-reported turnout by manually checking government records, the high cost of this validation procedure led to its discontinuation in the 1990s, making it difficult to resolve the controversy. Fortunately, Congress passed the Help America Vote Act in 2002, mandating that each state develop an official voter registration list. This enabled commercial firms to systematically collect and regularly update nationwide voter registration files (Ansolabehere and Hersh 2012). Both the ANES and CCES now rely on these commercial firms to validate the self-reported turnout.

Nevertheless, the debate about the causes of the bias of self-reported turnout rates persists. Most prominently, while Ansolabehere and Hersh (2012) use commercial validation for the 2008 CCES and find that overreporting is the culprit of bias in self-reported turnout, Berent, Krosnick, and Lupia (2011, 2016) analyze the 2008 ANES and contend that such findings are due to the poor quality of government records as well as the errors in matching survey respondents to registered voters in administrative records. Jackman and Spahn (2019) validate the self-reported turnout in the 2012 ANES by working with a commercial firm and relying on its proprietary method. They find that overreporting is responsible for six percentage points whereas nonresponse bias and inadvertent mobilization effects account for four and three percentage points, respectively. In sum, the existing evidence is mixed as to what biases self-reported turnout in public opinion surveys. Yet, these studies often rely on commercial validation, making it difficult to assess why their findings disagree with one another.

In this paper, we contribute to this literature by examining the validity of self-reported turnout in the 2016 US presidential election. Our validation study is based on both the ANES and CCES. We apply the canonical model of probabilistic record linkage, originally proposed by Fellegi and Sunter (1969) and recently improved by Enamorado, Fifield, and Imai (2019), to match survey respondents with registered voters in a nationwide voter file of more than 180 million records. Unlike Ansolabehere and Hersh (2012) and Jackman and Spahn (2019), who rely on a proprietary record linkage algorithm, we use the open-source software package fastLink (Enamorado, Fifield, and Imai 2017) to maximize the scientific transparency. In addition, unlike Berent, Krosnick, and Lupia (2016), who evaluated the performance of deterministic record linkage methods, we consider a probabilistic method that is more commonly used in the statistical literature (e.g., Lahiri and Larsen 2005; Winkler 2006). Our merge yielded public-use validated turnout data for the two surveys (Enamorado, Fifield, and Imai 2018a, 2018b). To the best of our knowledge, this paper describes the first effort to examine the empirical performance of a probabilistic record linkage method using large-scale administrative records in political science.

We find that the validated turnout rate for the ANES based on fastLink closely approximates the actual turnout rate when combined with clerical review.[2] For the CCES, the probabilistic record linkage method without clerical review yields the validated turnout rate close to the actual turnout rate. We conjecture that because the CCES is a noisier data set with many missing and invalid address entries, clerical review induces false negatives, thereby lowering a validated turnout rate. For both the ANES and CCES, similar validated turnout rates emerge for preelection and postelection surveys, suggesting that panel attrition accounts little for the bias in self-reported turnout. However, 30 to 40 percent of the matched nonvoters falsely report that they voted in the election, implying that overreporting is responsible for much of the bias. This finding agrees with the conclusion of Ansolabehere and Hersh (2012) but is inconsistent with that of Berent, Krosnick, and Lupia (2016). Similar to the previous literature, we find that those who are wealthy, partisan, highly educated, and interested in politics are more likely to overreport turnout. In addition, African Americans are more likely to overreport than other racial groups. Finally, with the CCES, the probabilistic record linkage method performs at least as well as the proprietary algorithm.

The Bias of Self-Reported Turnout Rates

The 2016 US presidential election provides an interesting and important case study for validating self-reported turnout rates. Donald Trump's surprising victory over Hillary Clinton contradicted most preelection forecasts and as a result raised the question of why polls failed (e.g., Kennedy et al. 2018). Researchers have suggested nonresponse and social desirability biases as possible explanations of polling inaccuracy (e.g., Enns, Lagodny, and Schuldt 2017), and these biases may also underlie the gap between self-reported and actual turnout rates. Hence, the validation exercise in this particular election should provide useful insights.

We begin our analyses by quantifying the bias of self-reported turnout rates obtained from the ANES and CCES. Along with turnout rates, we also examine self-reported registration rates.[3] The left three columns of table 1 present the self-reported turnout and registration rates of the ANES, while the fourth column shows the same results for CCES (standard errors that account for

[2] Clerical review refers to the process of human validation, focusing on those cases that are difficult for an automated algorithm to classify.

[3] Online Appendix A1.3 provides a detailed description of the question wordings and explains how each variable is coded.

Table 1. Comparison of the estimated turnout and registration rates based on self-reports and the administrative records for the 2016 US presidential election

	Self-reported				Administrative		
	ANES					Voter file	
	Overall	Face-to-face	Internet	CCES	Election Project	All	Active
Turnout rate (%)	76.0	78.0	75.3	83.8	58.8	57.5	
	(0.9)	(1.8)	(1.1)	(0.3)			
Registration rate (%)	89.2	89.2	89.2	91.9		80.4	76.6
	(0.7)	(1.2)	(0.9)	(0.2)			
Target population size (millions of voters)	224.1	222.6	224.1	224.1	232.4	227.6	227.6

NOTE.—Self-reported turnout and registration rates (with standard errors in parentheses) are obtained from the American National Election Studies (ANES) and Cooperative Congressional Election Study (CCES). Since the ANES has two modes of interview, face-to-face and internet, the estimated turnout and registration rates are computed separately for each mode as well as for the combined sample. The corresponding rates based on administrative records are computed using the voting-eligible population data from the United States Election Project and the nationwide voter file from L2, Inc. When using the voter file, we compute the registration rates in two ways, one based on all voters and the other based on active voters only. Each turnout rate has a slightly different target population, which is reflected by the differences in target population size.

survey designs are in parentheses).[4] For the ANES, we present the overall rates as well as the turnout and registration rates separately for the face-to-face and Internet samples. Note that the target population size differs only for the ANES face-to-face sample, which excludes those who reside in Alaska and Hawaii.[5]

We compare these self-reported rates with the corresponding rates based on the administrative records. We first compute the turnout rate among the voting eligible population (VEP) using the data from the United States Election Project. Since the target populations of ANES and CCES do not exclude individuals on parole or probation, we compute the actual turnout rates as the number of votes for the presidential race divided by the number of eligible voters plus the number of ineligibles minus the total number of prisoners. Unfortunately, we cannot adjust for overseas voters although they are excluded from the target population of both surveys. This is because no information exists about the number of votes cast by overseas voters. As a result, the VEP size has 8 to 10 million additional voters when compared to the target population of the two surveys. Thus, the actual turnout and registration rates presented here should be considered approximations. As noted earlier, the gap between self-reported and actual turnout rates is substantial, reaching 17 and 25 percentage points for the ANES and CCES, respectively.

Since our validation procedure involves merging survey data with a nationwide voter file, it is important to examine the accuracy of our specific voter file, or those who are recorded as casting a ballot for the presidential race. In July 2017, we obtained a nationwide voter file of over 180 million records from L2, Inc., a leading national nonpartisan firm and the oldest organization in the United States that supplies voter data and related technology to candidates, political parties, pollsters, and consultants for use in campaigns. While by then all states have updated their voter files by including the information about the 2016 election, in the routine data-cleaning processes by states and L2, some of the individuals who voted in the election might have been removed because they either have died or moved (based on the National Change of Address). As a result, the L2 voter file has a total of 131 million voters who cast their ballots whereas, according to the United States Election Project, approximately 136.7 million individuals voted in the election. In addition, the L2 voter file does not contain overseas voters, reducing the total VEP size by about 5 million and the turnout rate by slightly more than one percentage point.

Figure 2 compares state-level turnout rates based on the L2 voter file (horizontal axis) with their corresponding VEP turnout rates from the United States

[4] As described in detail in Online Appendix A1.1, the CCES is an opt-in survey with a nonprobabilistic sampling design. As such, the interpretation of its standard errors requires caution.

[5] Online Appendix A1.1 summarizes the sampling designs of the ANES and CCES and characterizes the target population of each survey and nonresponse problems. In addition, Online Appendix A1.1 describes the national voter file used in this paper and explain how it relates to the actual turnout rate and the target populations of the two surveys.

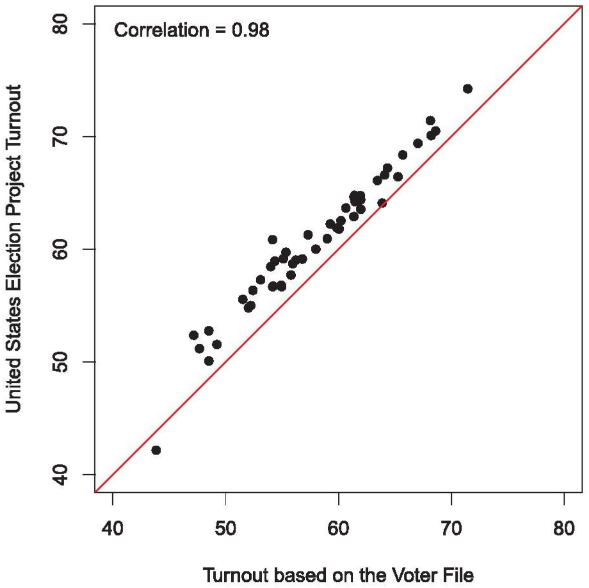

Figure 2. State-level comparison between the turnout rates (in percentage points) based on the voter file and the United States Election Project. The correlation between these turnout rates is high, and the average percentage point difference is small.

Election Project (vertical axis). While deceased voters and those who moved across states have been removed from the voter file, they are included in the VEP turnout calculation. As expected, the turnout rate based on the voter file is lower than the actual turnout. The median difference is 2.7 percentage points, whereas the standard deviation is one percentage point. However, the correlation between the two reaches 0.98. There also is a near-perfect correlation at the county level (see A2 of Online Appendix A2.1).

In addition, we compute the registration rate using the voter file. Since the voter file lists everyone who is registered to vote, we divide the total number of records in the voter file by the target population size. The voter file contains approximately 182 million records among a total of 8.6 million records that are classified by some states as "inactive voters." The definition of inactive voters differs from one state to another (and some states do not have such a classification), but they represent those who did not turn out in several consecutive elections and whom states were unable to contact. After being placed on the inactive voter list for a few years, these records will be purged by states. Typically, if inactive voters show up to vote at a polling station on an election day, they would have to provide proof of residence. This suggests that inactive voters may claim in a survey that they are not registered. Therefore, we compute the registration rate in two ways, one based on all records in the voter file and the other based on active voters alone. Similar to the self-reported turnout rates, the self-reported registration rates are much greater than those based on the voter file. The gap is about 10 percentage points if we use all records, whereas it is closer to 15 percentage points when the registration rate is based on active voters alone.

Finally, the magnitude of bias is much greater for these two election studies than the Voter Supplement of the Current Population Survey (CPS). Historically, the CPS has consistently produced self-reported turnout estimates that are closer to the actual turnout rates than the ANES. For example, for the past three general elections, the bias of the CPS self-reported turnout estimate has been at most three percentage points. Recently, some scholars have pointed out that the CPS treats those who dropped out or refused to answer the turnout question as nonvoters (Hur and Achen 2013). We leave to future research the question of whether (and if so why) the CPS yields more accurate self-reported turnout rates than the ANES and CCES (see DeBell et al. 2018).

Linking Surveys with Administrative Records

This section describes how the ANES and CCES were linked with the national voter file using the canonical model of probabilistic record linkage. Through research collaboration agreements with the ANES and YouGov, we obtained access to the deanonymized information for each of the 4,271 respondents (1,181 and 3,090 for the face-to-face and Internet samples, respectively) for the 2016 ANES as well as 64,600 respondents for the 2016 CCES. We used this information to link the survey data with the voter file.

PREPROCESSING NAMES AND ADDRESSES

As emphasized by Winkler (1995), a key step for a successful merge is to standardize the fields that will be used to link two datasets. Accordingly, every effort was made to parse the names and addresses used in the ANES and CCES uniformly so that their formats match with those of the corresponding fields in the nationwide voter file. For example, the full name of an individual is divided into the first, middle, and last names, while the address is parsed into house number, street name, zip code, and apartment number (see Online Appendix A1.2 for details).

The ANES makes use of data from the United States Postal Service to ensure that the invitation letter can be delivered to the sampled addresses. As a result, the ANES address data are of high quality. In contrast, the respondent names are self-reported and each name is represented by a string that we parsed into the first, middle, and last names. For self-reported registered voters, whenever available, we use the name, which they said they had used for registration

(3,623 records, or 85 percent). If no name was provided (either because an individual reported not having registered to vote or failed to provide a name; 464 records, or 11 percent), we use the name on a check sent as monetary compensation for their participation in the survey. For the remaining respondents, we use the names of individuals whom the ANES intended to interview (184 records, or 4 percent).

In the case of the CCES, both addresses and names are self-reported. Consequently, we parsed each name and address for all 64,600 respondents and made their format comparable to that of names and addresses in the nationwide voter file. In the case of names, a similar strategy as the one used for the ANES divided the name string into three components: first, middle, and last names. However, the names of almost 3 percent of respondents (1,748 individuals) were missing.

As noted, the CCES respondents self-report their address as well, and each of those addresses was stored as a single string variable. We first used the preprocText()function in fastLink to standardize each address according to the USPS Postal Address Information System.[6] This follows the same procedure used by the ANES to clean their sample of addresses. We then divided a standardized address into house number, street name, zip code, and apartment number. Unlike the ANES, which has no missing value, more than 7,000 records (or 11 percent) of the CCES respondents did not report their addresses.

Table 2 summarizes the results of preprocessing. The percentage of complete names across surveys is quite high, exceeding 90 percent for both surveys. The ANES has a higher proportion of complete names, regardless of its interview mode, than the CCES, which has some cases of missing names and uses of initials. However, there is an important difference in address fields between the two surveys. Since the ANES adopts the sampling design based on the list of residential addresses, all addresses are complete. In contrast, the CCES relies on the self-reported addresses by its respondents, resulting in the nonresponse rate of over 10 percent and some use of P.O. Box. Indeed, the CCES has 8,716 cases (13.5 percent of the preelection sample) without any information about names or a valid residential address. This makes it more challenging to merge the CCES data with the voter file.

MERGE PROCEDURE

Having standardized the linkage fields, we separately merge the ANES and CCES with the nationwide voter file. Since the nationwide voter file contains more than 180 million records, merging a survey data set with the voter file all at once would result in a total of over 756 billion and 18 trillion comparisons for the ANES and CCES, respectively. Therefore, we first subset the survey and voter file data into 102 blocks, defined by state of residence (50 states plus

[6] See https://pe.usps.com/cpim/ftp/pubs/pub28/pub28.pdf for more information.

Table 2. The data quality of the name and address fields for the 2016 ANES and 2016 CCES

	ANES						CCES	
	All		Face-to-face		Internet			
	Cases	%	Cases	%	Cases	%	Cases	%
Names								
Missing value (first or last name)	66	1.55	53	4.5	13	0.4	1748	2.7
Initials for first and last name	0	0.0	0	0.0	0	0.0	3274	5.1
Initials for first name but last name complete	16	0.4	7	0.0	9	0.3	506	0.8
Complete name	4,189	98.1	1,129	95.6	3,068	99.3	59,072	91.4
Addresses								
Missing value	0	0.0	0	0.0	0	0.0	7,465	11.5
P.O. Box	0	0.0	0	0.0	0	0.0	1,665	2.6
Complete address	4,271	100.0	1,181	100.0	3,090	100.0	55,470	85.9
Number of respondents	4,271		1,181		3,090		64,600	

Washington, DC) and gender (male and female). Thus, our merge procedure assumes gender is accurately measured for all voters. Once the within-state merge is done for each block, we conduct the across-state merge focusing on survey respondents who are not matched with registered voters through the within-state merge.

In the case of the ANES, the block size ranges from 48,315 pairs (Hawaii/Female: ANES = 3, Voter file = 16,105) to 705 million pairs (California/Female: ANES = 225, Voter file = 3,137,276) with the median value of 11 million pairs (Idaho/Male: ANES = 28, Voter file = 426,636). For the CCES, the block size ranges from more than 3 million (Wyoming/Male: CCES = 45, Voter file = 88,849) to 25 billion pairs (California/Male: CCES = 3073, Voter file = 8,326,559) with the median value of 301 million pairs (Iowa/Female: CCES = 394, Voter file = 764,169).

Within each block, we conduct the data merge using the following variables: first name, last name, age, house number, street name, and zip code. We apply the canonical model of probabilistic record linkage, originally proposed by Fellegi and Sunter (1969). Enamorado, Fifield, and Imai (2019) improved the implementation of the algorithm used to fit this model so that it is possible to merge large-scale data sets with millions of records. Throughout the merge process, we use the open-source package fastLink (Enamorado, Fifield, and Imai 2017) to fit the model to our data so that the procedure is transparent.

The model is fit to the data based on the agreement patterns of each linkage field across all possible pairs of records between the two data sets $\mathcal{A}$ and $\mathcal{B}$. We use three levels of agreement for the string valued variables (first name, last name, and street name) based on the Jaro-Winkler similarity measure with 0.85 and 0.94 as the thresholds (see, e.g., Winkler 1990).[7] We also use three levels of agreement for age based on the absolute distance between values, with 1 and 2.5 years as the thresholds used to separate agreements, partial agreements, and disagreements (see American National Election Studies [2016] for a similar choice). For the remaining variables (i.e., house number and postal code), we utilize a binary comparison indicating whether they have an identical value.

Formally, if we use a binary comparison for variable k, we define $\gamma_k(i,j)$ to be a binary variable, which is equal to 1 if record i in the data set $\mathcal{A}$ has the same value as record j in the data set $\mathcal{B}$. If the variable uses a three-level comparison, then we define $\gamma_k(i,j)$ to be a factor variable with three levels, in which 0, 1, and 2 indicate that the values of two records for this variable are different, similar, and identical, respectively.

[7] Jaro-Winkler is a commonly used similarity measure for strings. Unlike other alternative measures such as the Levenshtein distance and the Jaro similarity, the Jaro-Winkler similarity measure involves a character-wise comparison with a special emphasis on the first characters of the strings being compared.

Based on this definition, the record linkage model of Fellegi and Sunter (1969) can be written as the following two-class mixture model with the latent variable M_{ij}, indicating a match $M_{ij} = 1$ or a non-match $M_{ij} = 0$ for the pair (i,j),

$$\gamma_{\mathrm{k}}(i,j) \mid M_{ij} = m \overset{indep.}{\sim} Discrete(\boldsymbol{\pi}_{km}) \tag{1}$$

$$M_{ij} \overset{i.i.d.}{\sim} Bernoulli(\lambda) \tag{2}$$

where $\boldsymbol{\pi}_{km}$ is a vector of length L_k, which is the number of possible values taken by $\gamma_k(i,j)$, containing the probability of each agreement level for the kth variable given that the pair is a match ($m = 1$) or a non-match ($m = 0$), and λ. presents the probability of match across all pairwise comparisons. The model assumes (1) independence across pairs, (2) independence across linkage fields conditional on the latent variable M_{ij}, and (3) missing at random conditional on M_{ij} (Enamorado, Fifield, and Imai 2019). As shown in the literature (e.g., Winkler 1989, 1993; Thibaudeau 1993; Larsen and Rubin 2001), it is possible to relax this conditional independence assumption using the log-linear model (see Online Appendix A2.4 for the results based on this model).

Once the model is fit to the data, we estimate the probability of match using the Bayes rule based on the maximum likelihood estimates of the model parameters,

$$\xi_{ij} = Pr(M_{ij} = 1 | \boldsymbol{\delta}(i,j), \boldsymbol{\gamma}(i,j))$$

$$= \frac{\lambda \Pi_{k=1}^{K} \left(\Pi_{l=1}^{L_{k-1}} \pi_{k1l}^{1\{\gamma_k(i,j)=l\}} \right)^{1-\delta_k(i,j)}}{\sum_{m=0}^{1} \lambda^m (1-\lambda)^m \Pi_{k=1}^{K} \left(\Pi_{l=1}^{L_{k-1}} \pi_{kml}^{1\{\gamma_k(i,j)=l\}} \right)^{1-\delta_k(i,j)}} \tag{3}$$

where $\delta_k(i,j)$indicates whether the value of variable k is missing for pair (i,j) (a missing value occurs if at least one record for the pair is missing the value for the variable).

We say that record j is a potential match of record i if the estimated match probability ξ_{ij} is the largest among all pairs that involve record i. Formally, we define the following maximum estimated match probability for record i as follows:

$$\zeta_i = \max_{\mathrm{j} \neq \mathrm{i}} \xi_{ij} \tag{4}$$

If more than one record exists where the estimated match probability is equal to ζ_i, then we randomly select one of them as a match. Fortunately, in the current applications, there was no tie when ζ_i is reasonably high, for example, $\zeta_i \geq 0.75$, and hence random sampling has little effect. This procedure yields a one-to-one match for each respondent i with the estimated match probability of ζ_i.[8]

An important concern with our blocking strategy is that we may fail to match an individual whose residential address has changed between the day of the survey interview and the date when our voter file was updated. It is also possible that people were registered to vote in a residential address different from the address they reported in the surveys. To identify these individuals, we take all survey respondents whose estimated match probability ζ_i is less than 0.75 and then merge them with registered voters in other states. There exists a total of 1,100 such respondents for the ANES and 23,585 respondents for the CCES.

To conduct this across-state merge, we first subset the nationwide voter file such that it only contains the registered voters whose names are close to the remaining survey respondents. As before, we use the Jaro-Winkler string distance of 0.94 or above as the threshold. This reduces the number of registered voters from over 180 million to 14 million. Using fastLink, we find, for each survey respondent, a registered voter who has the same name (first, middle, and last) and the identical age where the names with the Jaro-Winkler distance of 0.94 or above are coded as the same. This yields 51 and 874 additional matches for the ANES and CCES, respectively, and for these matches the estimated match probability is close to 1.[9] For those respondents who are not matched, we use the matches from the within-state merge.[10]

As an optional final step, we conduct a clerical review (human validation) of each respondent, which is recommended by some in the literature (e.g., Winkler 1995), and set the estimated match probability to zero for those respondents who, our clerical review suggests, do not have a valid match. We caution that a clerical review may not be useful when the data contain many missing or mismeasured variables. In such cases, a clerical review may increase false negatives while reducing false positives. In our applications, as shown in Table 2, the names and addresses are more complete for the ANES than for the CCES. As a result, a clerical review may be more appropriate for the ANES.

Our clerical review discards 284 (8.7 percent of matches) and 4,115 (9.6 percent of matches) records as matches for the ANES and CCES, respectively.

[8] We examine the robustness of our results by conducting one-to-many matching strategy as described in Enamorado, Fifield, and Imai (2019). Specifically, we compute the weighted average of all matched turnout records using the normalized weights that are proportional to the estimated match probabilities. The results are presented in table A1 of Online Appendix A2.2 and are essentially identical to the results based on one-to-one match.

[9] Recently, Goel et al. (2019), using synthetic data, found that a merge based just on names and date of birth via fastLink is able to identify duplicated records across different geographic units with a high degree of precision, even in the presence of measurement error in the linkage fields.

[10] Figure A3 of Online Appendix A2.3 presents the distributions of the estimated match probabilities for the ANES and CCES.

For example, 124 cases in the ANES and 2,335 in the CCES are removed because each of them is matched with an individual in the same household who has the same name but also has an age difference of more than 5 years and/or does not share a single component of birthday (day, month, or year). This suggests that these matched individuals are likely to be relatives. Similarly, we discard 39 cases in the ANES and 59 in the CCES, where matched individuals have the same name and age, but a different address and middle name. Finally, we remove 60 cases in the ANES and 1,404 in the CCES where individuals had the same address and age, but the names were completely different.

ESTIMATED MATCH RATES

To summarize the results of the merge, we estimate the overall match rate as $\sum_{i=1}^{N} \zeta_i / N$ where N is the total number of survey respondents.[11] Table 3 presents the match rates for the ANES and CCES using the preelection and postelection survey respondents. For the ANES, we present the match rate separately for the face-to-face and Internet samples as well as for the combined sample ("Overall"). The results are based on the probabilistic model alone ("fastLink") and the model plus clerical review ("clerical review").

For the sake of comparison, we also present the two estimates of registration rate based on the voter file for the target populations for surveys. The first ("all") is the total number of voters in the voter file divided by the number of eligible voters. However, these registration rates are likely to overestimate the true rates because some voters may be deceased or have moved. For this reason, as explained earlier, in some (but not all) states, the Secretary of State office labels voters "inactive" before purging them from the voter file. The second estimate ("active") uses the total number of active voters as the numerator. Since the exact definition of active voters varies by states and some states do not distinguish active and inactive voters, these estimates may not approximate the actual registration rate. It is possible that survey respondents may think they are registered even though they are classified as inactive voters or even removed from the voter file. In the final column, we also present the estimated registration rate based on self-reports from the CPS.

For the ANES, the match rates based on the probabilistic model alone ("fastLink") are similar to the registration rates based on active voters. After the clerical review, however, the estimates become closer to the self-reported registration rates from the CPS. There is little difference in results between the preelection and postelection samples as well as between the interview mode. For the CCES, the match rates are generally lower than those of the ANES. This makes sense since the CCES contains a larger number of missing and misreporting entries for names and addresses. For the noisy data like the CCES,

[11] This assumes one-to-one match. Online Appendix A2.2 relaxes this assumption and presents the results based on one-to-many matches.

Table 3. Estimated match rates from the results of merging the ANES and CCES with the nationwide voter file

		Preelection		Postelection		Registration rate		
						Voter file		
		fastLink	Clerical review	fastLink	Clerical review	All	Active	CPS
ANES	Overall	76.5	68.8	77.2	69.9	80.4	76.6	70.3
		(0.6)	(0.7)	(0.7)	(0.8)			(1.4)
	N	4,271		3,649				
	Internet	77.0	69.2	77.8	70.2	80.4	76.6	70.3
		(0.7)	(0.8)	(0.8)	(0.9)			(1.4)
	N	3,090		2,590				
	Face-to-face	75.3	67.8	75.6	69.1	80.2	76.4	70.4
		(1.2)	(1.4)	(1.3)	(1.4)			(1.4)
	N	1,181		1,059				
CCES		66.6	58.6	70.5	63.6	80.4	76.6	70.3
		(0.2)	(0.2)	(0.2)	(0.2)			(1.4)
	N	64,600		52,899				

NOTE.—For the ANES, we compute the match rates separately for the face-to-face and internet samples as well as together for the overall sample. Merging is based on the probabilistic model alone ("fastLink") and the model plus clerical review ("clerical review"). Standard errors are in parentheses. For the sake of comparison, we also present the estimated registration rates from the voter files (all registered voters "all" and active voters only "active") as well as the self-reported registration rate from the Current Population Survey (CPS). Each registration rate is computed for the target population of corresponding survey estimate.

probabilistic models alone might perform better because clerical review may end up with a greater number of false non-matches while reducing false positives. Finally, for the CCES, the match rate for the preelection sample is about four to five percentage points lower than those for the postelection sample. This suggests that unlike the ANES, the weighting adjustment may not be sufficient to adjust for attrition in the CCES.

Merging the 2008 ANES respondents with the voter files for six states, Berent, Krosnick, and Lupia (2016) find that the match rates are significantly lower than the registration rates. The authors use this as evidence to argue that the validated turnout rates are lower than self-reported turnout rates not because survey respondents overreport but because merging methods fail to match some respondents who voted with voter registration records. A similar pattern emerges: The match rates based on our probabilistic approach are generally lower than the registration rates based on the voter file. However, as explained above, the registration rates based on the voter file are likely to overestimate the true rates because of inactive voters who remain in the voter file. Thus, our interpretation of this result differs from that of Berent, Krosnick, and Lupia (2016). Below, we present evidence that overreporting is primarily responsible for the bias in self-reported turnout.

Results

This section presents the results of our turnout validation. We begin by showing validated turnout rates, then examine the potential sources of bias in self-reported turnout rates. Finally, we identify the types of voters who tend to overreport their turnout and compare our validation results with those of a commercial vendor.

VALIDATED TURNOUT RATES

To obtain the validated turnout rate, we compute the weighted average of the binary turnout variable among matched voters in the voter file where the estimated match probability ζ_i is used as the (unnormalized) weight. Table 4 presents the validated turnout rates among the survey respondents from the preelection and postelection surveys of the 2016 ANES and CCES. As in table 3, we compare the results obtained from the probabilistic model alone ("fastLink") and the model plus clerical review ("clerical review") with actual turnout rates based on the voter file ("Voter file") and the United States Election Project ("Election Project"). The standard errors that account for sampling design and unit nonresponse are given in parentheses.

Our main findings about turnout rates are consistent with those about registration rates given in table 3. For the ANES, the validated turnout rates directly obtained from fastLink are at least five percentage points greater than the

Table 4. Validated turnout rates among the survey respondents from the 2016 ANES and CCES

		Preelection		Postelection		Actual turnout	
		fastLink	Clerical review	fastLink	Clerical review	Voter file	Election Project
ANES	Overall	63.6	58.1	65.0	59.8	57.6	58.8
		(0.9)	(0.9)	(1.0)	(1.0)		
	N	4,271		3,649			
	Internet	62.6	57.4	64.0	58.6	57.6	58.8
		(1.1)	(1.1)	(1.2)	(1.2)		
	N	3,090		2,590			
	Face-to-face	66.5	61.1	67.6	63.1	57.6	58.9
		(1.8)	(1.8)	(1.7)	(1.8)		
	N	1,181		1,059			
CCES		54.1	48.5	55.7	50.3	57.6	58.8
		(0.3)	(0.3)	(0.4)	(0.4)		
	N	64,600		52,899			

NOTE.—The validated turnout rates obtained from the probabilistic model alone ("fastLink") and the model plus clerical review ("clerical review") are compared to the actual turnout rate for the corresponding target population based on the voter file and the data from the United States Election Project. The standard errors are in parentheses.

actual turnout rates. However, clerical review helps close this gap, yielding the validated turnout rates that are within the sampling error of the actual turnout rates. For the sample of face-to-face interviews, the validated turnout rates are higher than the internet sample, though the standard errors are greater.[12]

For the CCES, the validated turnout rates directly obtained from fastLink are closer to the actual turnout rates than those based on the model and clerical review. The reason for this difference is the same as the one discussed earlier. Because the CCES contains many misreported and missing entries especially for addresses, clerical review ends up removing the potential matches involving these records and hence introducing false negatives. This suggests that clerical review may be ineffective for noisy data. We also note that the validated turnout rates based on the model and clerical review are similar to the result obtained by YouGov based on a voter file provided by a commercial firm, Catalist.

We conduct several robustness checks. First, we compare the results based on one-to-one matching strategy with those based on one-to-many matching strategy described in Enamorado, Fifield, and Imai (2019). Table A1 of Online Appendix A2.2 shows that these results are essentially identical. Second, Online Appendix A2.4 presents the results from the log-linear model that does not require the conditional independence assumption. Although the substantive results are similar, the resulting matched and validated turnout rates are somewhat lower than those obtained under the conditional independence assumption. Finally, Online Appendix A3 further compares our validated turnout with that based on the vote validation conducted for the CCES using data from Catalist and a proprietary algorithm. Overall, we find that fastLink performs at least as well as a state-of-the-art proprietary algorithm (see Online Appendix A3 for more details).

POSSIBLE SOURCES OF BIAS IN SELF-REPORTED TURNOUT

What are the possible sources of differences between self-reported and validated turnout rates? The literature suggests overreporting, attrition, and mobilization as the main culprits. Below, we show that overreporting accounts for more than 90 percent of the bias of self-reported turnout, while nonresponse due to attrition plays a smaller role. Unfortunately, unlike Jackman and Spahn (2019), we cannot examine the contribution of mobilization to the bias of self-reported turnout because of a design difference between the 2012 and 2016 ANES face-to-face surveys.

Misreporting. We first consider overreporting as a potential source of bias in self-reported turnout. Table 5 presents the validated turnout rates among survey respondents with different responses to the turnout questions of the

[12] See Online Appendix A1.4 for more details about the different sampling weights of the ANES and CCES.

Table 5. Validated turnout rates among survey respondents with different responses to the turnout questions in the ANES and CCES

			Registered		Postelection
		Not registered	Did not vote	Voted	attrition
ANES	fastLink	8.1	14.5	81.7	55.7
		(1.6)	(1.7)	(0.9)	(2.4)
	Clerical review	0.9	6.0	77.4	48.3
		(0.8)	(1.2)	(1.0)	(2.4)
	N	390	481	2,770	629
		(26.0)	(27.0)	(61.8)	(29.3)
CCES	fastLink	16.4	10.2	73.1	24.0
		(0.8)	(0.7)	(0.3)	(0.6)
	Clerical review	8.0	4.7	68.7	16.4
		(0.7)	(0.6)	(0.3)	(0.5)
	N	10,324	1,096	41,561	11,565
		(211.2)	(30.5)	(218.1)	(194.4)

NOTE.— "Postelection attrition" refers to the group of survey respondents who did not answer the turnout questions due to attrition. Standard errors that account for the sampling designs and unit nonresponse are in parentheses.

ANES and CCES. About 20 percent of the ANES respondents who said they had voted in the postelection survey did not turn out according to the voter file, whereas the corresponding estimated proportion of overreporting for the CCES is about 30 percent. Compared to the probabilistic model alone ("fastLink"), the use of clerical review ("clerical review") increases the estimated overreporting rate by several percentage points for both surveys. Because a majority of respondents said they had voted (78 percent for the ANES and 85 percent for the CCES), overreporting is mostly responsible for the upward bias in self-reported turnout.

In terms of underreporting, the results from fastLink show that approximately 69 voters, or 15 percent (109 voters, or 10 percent), of the ANES (CCES) respondents who said they had registered but had not voted were matched with registered voters who had voted in the 2016 election. Once the clerical review is conducted, this number is reduced to 29 voters, or 6 percent (49 voters, or 4 percent). In addition, 32 voters, or less than 9 percent (1,690 voters, or 16 percent), of the ANES (CCES) respondents who said they had not registered actually turned out in the election according to the matched voter records. Again, clerical review reduces this number to 3 voters, or less than 1 percent (830 voters or 8 percent), of the ANES (CCES) respondents. These discrepancies, while smaller, represent potential misreporting that may contribute to a downward bias. However, among the validated voters (i.e., those who are at risk of underreporting), at most only 1.3 percent (2.8 percent) of the ANES (CCES) respondents are found to have underreported.[13] Therefore, we conclude that potential underreporting contributes little to the bias of the overall self-reported turnout rates.

Table 6 provides additional evidence that survey respondents tend to overreport turnout. It presents the self-reported turnout rates among the survey respondents who are matched with registered voters in the voter file. For the results based on fastLink without clerical review, we use the estimated match probability as described in equation (4) to weight each observation.

Although misreporting is almost nonexistent among those who are validated to have voted, more than 30 percent (40 percent) of the validated nonvoters of the ANES (CCES) self-reported to have voted in 2016. This finding is consistent with that of Ansolabehere and Hersh (2012). While matched nonvoters may differ from nonvoters who are not matched, our finding suggests that the unmatched nonvoters may also overreport their turnout, leading to a substantial overreporting. Our finding contradicts the claim put forth by Berent, Krosnick, and Lupia (2016) that survey respondents do not often overreport turnout. These authors show that matched respondents tend not to overreport. However, they did not separate matched voters from matched nonvoters, and as a result overlooked the tendency of matched nonvoters to overreport.

Table 6. Self-reported turnout rates among matched voters and nonvoters

		Voters		Nonvoters		
		%	Cases	%	Cases	Total
ANES	FastLink	95.7 (0.5)	2,436	33.7 (3.0)	378	2,814
	Clerical review	98.5 (0.3)	2,258	30.8 (3.5)	290	2,548
CCES	FastLink	92.7 (0.4)	33,329	43.5 (1.3)	3,901	37,230
	Clerical review	96.3 (0.3)	30,741	44.4 (1.8)	2,836	33,577

NOTE.—The "Voters" ("Nonvoters") column presents the self-reported turnout rate among the survey respondents who are validated to have voted (have abstained) in the 2016 election. More than 30 percent (40 percent) of the ANES (CCES) survey responded who did not vote reported that they had voted. Standard errors are in parentheses.

[13] In this analysis, underreporting is defined as a binary variable that equals one if a respondent who said he/she did not vote is matched with a registered voter who turned out.

Attrition: The consequences of attrition are reflected in the last column of table 5, which presents the validated turnout among those who dropped out after the preelection survey and did not answer the postelection survey. The validated turnout rate for the ANES dropouts is similar to the overall turnout, suggesting that attrition does not substantially bias the results. For the CCES, those who did not answer the postelection survey have a much lower validated turnout rate, implying that attrition may have contributed to the bias of self-reported turnout.

This pattern is consistent with table 4, which shows the similarity of the validated turnout rates between the preelection and postelection surveys for the ANES, but not for the CCES. In contrast with some previous work in the literature (e.g., Burden 2000), this finding suggests that attrition is unlikely to explain the gap between the self-reported and actual turnout rates for the ANES, though it may be responsible for some, but not all, of the bias for the CCES. Sampling weights of the ANES appear to be able to properly adjust for the possible bias due to unit and item nonresponse.[14]

WHO OVERREPORTS TURNOUT?

To determine who overreports, we conduct a regression analysis using the sample of validated nonvoters alone. The outcome variable is binary and equals one if a respondent self-reported that she voted but our turnout validation based on fastLink and clerical review found that she did not. The weighted logistic regression model with survey weights includes several covariates used in the literature (e.g., Ansolabehere and Hersh 2012, and references therein): age, marital status, highest level of educational attainment, gender, race, income, partisanship, religiosity, and ideology. Online Appendix A1.5 explains the coding rules used to harmonize covariates across the two surveys to facilitate the comparison of the results. Since underreporting does not appear to be problematic in both surveys (less than 1 percent of the postelection respondents for both the ANES and CCES), we focus on the analysis of overreporting rather than underreporting.

Following the literature on overreporting (e.g., Silver, Anderson, and Abramson 1986; Belli, Traugott, and Beckmann 2001; Bernstein, Chadha, and Montjoy 2001; Deufel and Kedar 2010; Ansolabehere and Hersh 2012), we examine the sample of validated nonvoters only, which includes those respondents classified as nonvoters in the 2016 presidential election by fastLink and clerical review (1,390 and 21,835 respondents for the ANES and CCES, respectively). Figure 3 presents the estimated proportions of overreports among the validated nonvoters across the different values of some covariates,

[14] Online Appendix A5 shows that a merge based on the address information alone leads to a similar conclusion. This suggests that for turnout and registration, the preelection and postelection samples are not different from each other.

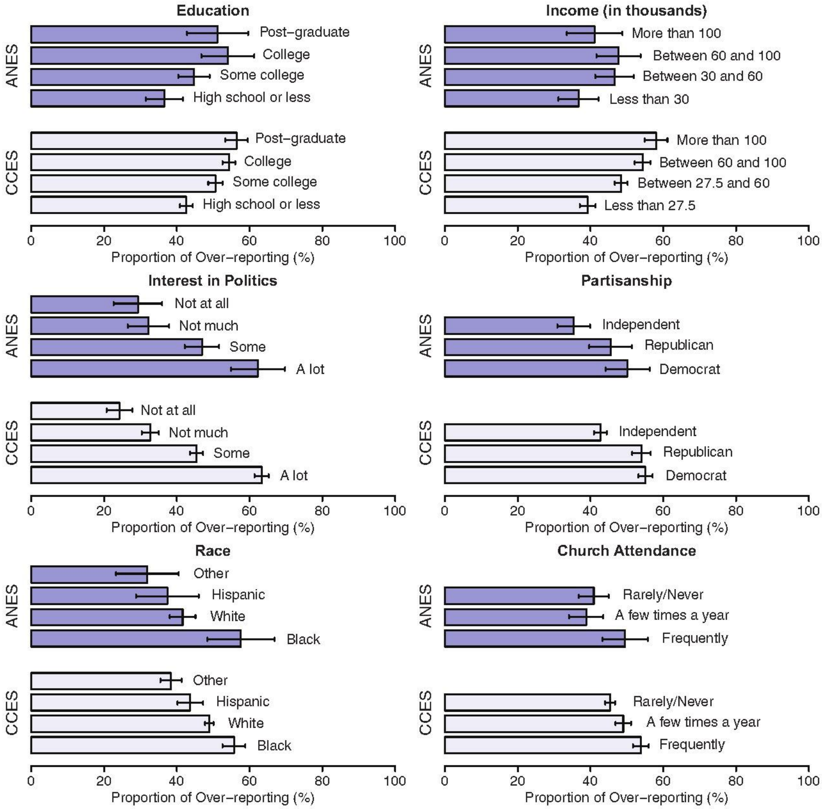

Figure 3. Estimated proportion of overreporting across different covariates in the sample of validated nonvoters. The results are based on the weighted logistic regression separately fitted to the CCES (light blue) and ANES (dark blue) samples of validated nonvoters. Each plot presents the estimated proportion of overreporting averaging over the entire sample of validated nonvoters while fixing the other covariates at their observed values. Nonresponse is treated as a separate category for each covariate.

whose coefficients are estimated to be statistically significantly different from zero. These estimates are obtained by averaging over all respondents in the sample of validated nonvoters (using the sampling weights) while fixing the other covariates to their observed values. Thus, each estimated regression coefficient represents the predicted difference in overreporting between two individuals who share all the observed characteristics except the corresponding covariate.

The figure graphically summarizes the results, while the estimated coefficients and their standard errors are given in table A4 of Online

Appendix A2.5.[15] For both the ANES and CCES, we find similar patterns: Educated respondents tend to overreport more than the uneducated, partisans are more likely to overreport than independents, and those who said they were interested in politics overreport more than those with little interest.[16] Although the overall pattern is similar between the two surveys, there are some small differences. For example, for the CCES, a monotonic relationship exists between income and overreporting: Respondents with high income tend to overreport more than poor respondents. However, for the ANES, the relationship is not monotonic. In addition, for the ANES, a substantial difference emerges in the propensity to overreport turnout between African Americans and the other voters, whereas the magnitude of this difference is much smaller for the CCES.

These results are in line with the findings of other validation studies that have used ANES data and proprietary record linkage algorithms. For example, previous studies have found that those who are more partisan (e.g., Ansolabehere and Hersh 2012), interested in politics (e.g., Bernstein, Chadha, and Montjoy 2001; Ansolabehere and Hersh 2012), educated (e.g., Bernstein, Chadha, and Montjoy 2001; Ansolabehere and Hersh 2012), and wealthier (e.g., Ansolabehere and Hersh 2012) are more likely to overreport turnout. In addition, our findings are consistent with the existing studies that show African Americans are more likely to overreport if compared to other racial groups (e.g., Traugott and Katosh 1979; Abramson and Claggett 1992; Belli, Traugott, and Beckmann 2001; Bernstein, Chadha, and Montjoy 2001; Deufel and Kedar 2010). However, unlike some older studies such as Silver, Anderson, and Abramson (1986) and Bernstein, Chadha, and Montjoy (2001), our results do not show a strong relationship between overreporting and age, and overreporting and religiosity. These discrepancies may arise in part because the nature of overreporting may have possibly changed over time. Additional validation studies are needed to further investigate these differences.

Conclusion

Over the last decade, the availability of large-scale electronic administrative records has enabled researchers to study important questions by creatively

[15] Online Appendix A2.6 presents a bivariate analysis of overreporting. We focus on two outcomes, the proportion and the odds ratio of overreporting for the different values taken by each covariate commonly used to explain who is more likely to overreport. The bivariate analysis recovers the patterns similar to the ones obtained by the multivariate regression analysis (see tables A6 and A7).

[16] In addition, table A5 of Online Appendix A2.5 presents the results concerning the determinants of overreporting for the ANES sample separately for each interview mode. The patterns observed using the complete sample are quite similar to those obtained by focusing on the face-to-face and internet samples of the ANES.

merging them with other data sets (e.g., Jutte, Roos, and Brownell 2011; Ansolabehere and Hersh 2012; Einav and Levin 2014). A major methodological challenge of these studies, however, is that there often exists no unique identifier that can be used to unambiguously merge data sets. In these situations, probabilistic record linkage methods that have been developed in the statistics literature over the last several decades can serve as a useful methodological tool.

This paper presents a case study that applies the canonical record linkage method of Fellegi and Sunter (1969) to merge two prominent national election survey data sets with the nationwide voter file of more than 180 million records. We show that the recent computational improvements make it possible to conduct this large-scale data merge. Unlike the previous studies, which relied upon proprietary algorithms, we use the newly developed open-source software package, facilitating the transparency, replicability, and falsifiability of scientific studies. Our analysis demonstrates that the probabilistic record linkage method can successfully validate turnout and shed light on the debate regarding the potential causes of bias in self-reported turnout. The probabilistic method is especially effective dealing with missing and invalid entries, as shown in the case of the CCES validation. We believe that a similar application of probabilistic record linkage methods in other domains can also be fruitful, leading to new scientific discoveries.

Finally, an important implication is that when designing surveys one could anticipate the potential difficulties that arise while merging survey data with administrative records. In particular, one could maximize the accuracy of measurements that are used for linking records. For example, the complete address records of the ANES played an important role in its successful turnout validation. In addition, if a survey has multiple ways like the ANES and CCES, one could merge the first wave and verify the necessary information in subsequent waves.

Supplementary Data

Supplementary data are freely available at *Public Opinion Quarterly* online.

References

Abramson, Paul R., and William Claggett. 1992. “The Quality of Record Keeping and Racial Differences in Validated Turnout.” *Journal of Politics* 54:871–80.

American National Election Studies. 2016. “*User’s Guide and Codebook for the ANES 2012 Time Series Voter Validation Supplemental Data.*” Technical Report, University of Michigan and Stanford University, Ann Arbor and Palo Alto, CA.

Ansolabehere, Stephen, and Eitan Hersh. 2012. “Validation: What Big Data Reveal about Survey Misreporting and the Real Electorate.” *Political Analysis* 20:437–59.

Belli, Robert F., Michael W. Traugott, and Matthew N. Beckmann. 2001. "What Leads to Voting Overreports? Contrasts of Overreporters to Validated Voters and Admitted Nonvoters in the American National Election Studies." *Journal of Official Statistics* 17:479–98.

Berent, Matthew K., Jon A. Krosnick, and Arthur Lupia. 2011. "*The Quality of Government Records and 'Over-Estimation' of Registration and Turnout in Surveys: Lessons from the 2008 ANES Panel Study's Registration and Turnout Validation Exercises*." Technical Report nes012554, American National Election Studies, Ann Arbor, MI, and Palo Alto, CA.

———. 2016. "Measuring Voter Registration and Turnout in Surveys." *Public Opinion Quarterly* 80:597–621.

Bernstein, Robert, Anita Chadha, and Robert Montjoy. 2001. "Overreporting Voting: Why It Happens and Why It Matters." *Public Opinion Quarterly* 65:22–44.

Burden, Barry C. 2000. "Voter Turnout and the National Election Studies." *Political Analysis* 8:389–98.

DeBell, Matthew, Jon A. Krosnick, Katie Gera, David S. Yeager, and Michael P. McDonald. 2018. "The Turnout Gap in Surveys: Explanations and Solutions." *Sociological Methods & Research* doi:10.1177/0049124118769085.

Deufel, Benjamin J., and Orit Kedar. 2010. "Race and Turnout in U.S. Elections Exposing Hidden Effects." *Public Opinion Quarterly* 74:286–318.

Einav, Liran, and Jonathan Levin. 2014. "Economics in the Age of Big Data." *Science* 346. doi:10.1126/science.1243089.

Enamorado, Ted, Benjamin Fifield, and Kosuke Imai. 2017. fastlink: Fast probabilistic record linkage. Available at Comprehensive R Archive Network (CRAN). https://cran.r-project.org/web/packages/fastLink/index.html.

———. 2018a. "*User's Guide and Codebook for the ANES 2016 Time Series Voter Validation Supplemental Data*." Technical Report, American National Election Studies.

———. 2018b. "*User's Guide and Codebook for the CCES 2016 Voter Validation Supplemental Data*." Technical Report, Cooperative Congressional Election Study.

———. 2019. "Using a Probabilistic Model to Assist Merging of Large-Scale Administrative Records." *American Political Science Review* 113:353–71.

Enns, Peter K., Julius Lagodny, and Jonathan P. Schuldt. 2017. "Understanding the 2016 US Presidential Polls: The Importance of Hidden Trump Supporters." *Statistics, Politics, and Policy* 8:41–63.

Fellegi, Ivan P., and Alan B. Sunter. 1969. "A Theory of Record Linkage." *Journal of the American Statistical Association* 64:1183–1210.

Goel, Sharad, Marc Meredith, Michael Morse, David Rothschild, and Houshmand Shirani-Mehr. 2019. "*One Person, One Vote: Estimating the Prevalence of Double Voting in U.S. Presidential Elections*." Technical Report, University of Pennsylvania.

Hur, Aram, and Christopher H. Achen. 2013. "Coding Voter Turnout Responses in the Current Population Survey." *Public Opinion Quarterly* 77:985–93.

Jackman, Simon, and Bradley Spahn. 2019. "Why Does the American National Election Study Overestimate Voter Turnout?" *Political Analysis* 27:193–207.

Jutte, Douglas P., Leslie L. Roos, and Marni D. Brownell. 2011. "Administrative Record Linkage as a Tool for Public Health Research." *Annual Review of Public Health* 32:91–108.

Kennedy, Courtney, Mark Blumenthal, Scott Clement, Joshua D. Clinton, Claire Durand, Charles Franklin, Kyley McGeeney, et al. 2018. "An Evaluation of the 2016 Election Polls in the United States." *Public Opinion Quarterly* 82:1–33.

Lahiri, Partha, and Michael D. Larsen. 2005. "Regression Analysis with Linked Data." *Journal of the American Statistical Association* 100:222–30.

Larsen, Michael D., and Donald B. Rubin. 2001. "Iterative Automated Record Linkage Using Mixture Models." *Journal of the American Statistical Association* 96:32–41.

McDonald, Michael P., and Samuel L. Popkin. 2001. "The Myth of the Vanishing Voter." *American Political Science Review* 95:963–74.

Silver, Brian D., Barbara A. Anderson, and Paul R. Abramson. 1986. "Who Overreports Voting?" *American Political Science Review* 80:613–24.

Thibaudeau, Yves. 1993. "The Discrimination Power of Dependency Structures in Record Linkage." *Survey Methodology* 19:31–38.

Traugott, Michael W., and John P. Katosh. 1979. "Response Validity in Surveys of Voting Behavior." *Public Opinion Quarterly* 43:359–77.

Winkler, William E. 1989. "Near Automatic Weight Computation in the Fellegi-Sunter Model of Record Linkage." Technical Report, Proceedings of the Census Bureau Annual Research Conference.

———. 1990. "String Comparator Metrics and Enhanced Decision Rules in the Fellegi-Sunter Model of Record Linkage." Proceedings of the Section on Survey Research Methods. American Statistical Association.

———. 1993. "Improved Decision Rules in the Fellegi-Sunter Model of Record Linkage." Proceedings of Survey Research Methods Section, American Statistical Association.

———. 1995. "Matching and Record Linkage." In *Business Survey Methods*, 355–84. New York: J. Wiley.

———. 2006. *Overview of Record Linkage and Current Research Directions*. Technical Report, U.S. Bureau of the Census.